

## Comment on wcd-2021-14

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Community comment on "Emergence of representative signals for sudden stratospheric warmings beyond current predictable lead times" by Zheng Wu et al., Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2021-14-CC1>, 2021

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This is an interesting paper that does a nice job investigating the relative role of linear and nonlinear processes for driving stratospheric variability, with important implications for subseasonal predictability. In particular, I think it is interesting that the results suggest that linear processes likely dominate downward propagating stratospheric anomalies until the final week or so prior to an SSW, at which point nonlinear processes seem to become important. To be clear, my comments below are not really criticisms of your results, rather I am just trying to mention a few issues that I think would be useful to keep in mind when interpreting what your results imply both in terms of physical process understanding and subseasonal predictability. (*Note, all references that I cite are listed at the bottom of this review*)

To begin, I should point out that I think that the overall methodology of employing EOFs as a filter is, for the most part, totally fine. Indeed, in many respects, an 'EOF-filter' is a better filter technique than a spectral filter because it implicitly takes into account both spatial and temporal information. That said, I think that a little bit of caution needs to be used when interpreting what a single EOF represents (here I am mostly, though not completely, referring to your 'SSW-EOF',  $E_1$ ).

As nicely discussed in Monahan et al. (2009, in particular their Sect. 3), individual EOFs generally do not coincide with the dynamical/physical modes of the dynamical system from which they are derived. One reason for this, is that the atmosphere and ocean (and certainly the coupled atmospheric-oceanic system) contain processes with very different timescales, which results in a 'nonnormal' dynamical system. This concept (i.e., 'nonnormality') is relatively easy to understand in terms of something like the NAO, where distinct physical processes like ENSO, the MJO, synoptic-eddy feedbacks, and SSWs all project onto the NAO pattern. This means that total NAO variance (as represented by the 1<sup>st</sup> EOF of Atlantic MSLP or geopotential) is not a single physical/dynamical mode, but rather a convolution of variance that arises due to many individual physical processes/dynamical processes. That is, any given NAO anomaly is a result of constructive/destructive interference between the different types of variability (i.e., ENSO, MJO, etc.).

Now, thinking in terms of nonnormality has important implications for your results, because (1) it is important for interpreting what physical processes may give rise to your

$E_1$  EOF, and (2) it will dictate what portion of the  $E_1$  EOF variance might be predictable at various forecast leads. To help explain what I mean by this, below I use one possible scenario as an example (the PJO/NAM). To be clear, I do not know to what extent this scenario is applicable to your results (though I would guess it is), but the scenario itself is perhaps less important than thinking about what nonnormality means in terms of your  $E_1$  EOF and the predictability of the variance that this EOF represents.

The potential scenario that I consider here is that your  $E_1$  EOF represents a broader class of stratospheric variability that regularly occurs and is occasionally punctuated by an SSW event. This is an idea envisioned by several previous authors (e.g., Kodera et al. 2000 and Kuroda and Kodera 2004) where it is postulated that the polar night jet oscillation (PJO) can be considered to be a general class of downward propagating stratospheric anomalies, that may occasionally be punctuated by a particularly strong PJO-like event in the form of a SSW. Because the PJO is typically identified via EOFs, it therefore likely arises from the nonnormal dynamics of ENSO, the MJO, the QBO, internal variability etc. Obviously some of these processes are more predictable at subseasonal leads than others. The SSW on the other hand, may typically (though not always) be due to internal nonlinear stratospheric dynamics (e.g., Sjöberg and Birner 2014, Birner and Albers 2017, White et al. 2019, de la Cámara 2019, Nakamura et al. 2020), which are unlikely to be predictable beyond 1-2 weeks (i.e., variability governed by the deterministic limit of predictability).

So, what does this mean in terms of your results and subseasonal predictability? One possibility is that the portion of  $E_1$  that you find behaves linearly is part of a broader class of PJO or NAM like variability that is *not necessarily* indicative of a future SSW, but may, under certain circumstances, be predictable on subseasonal timescales as you have suggested. On the other hand, the nonlinear portion of  $E_1$  may *necessarily* be related to SSWs *only*, and may never be predictable beyond synoptic forecast leads (1-2 weeks). Complicating matters further, is the open question of whether PJO/NAM events without a SSW are strong enough to generate predictable anomalies in the troposphere.

Overall, I think that it is probably important for you to comment in your paper on what can (or cannot) conclusively be physically implied about what your  $E_1$  EOF represents. For example, what processes might give rise to the potentially predictable (linear) behavior that you have identified 25 days prior to SSW onset? Is the SSW a culmination of those linear processes that somehow transition to nonlinear behavior (for e.g., finite amplitude ideas such as Nakamura et al. 2020 or resonance of some kind)? Or does the nonlinear behavior occur independently from the linear processes? In addition, in the future, it would be useful to determine whether  $E_1$ -type variability that occurs with or without a SSW might imply different levels of enhanced tropospheric subseasonal skill. In other words, if an SSW is required in order to make the stratospheric anomaly large enough to be associated with enhanced tropospheric forecast skill, but the SSW is ultimately only predictable 1-2 weeks ahead of time, then does that mean that the weaker stratospheric anomalies that are linearly predictable at 3-4 week leads may unfortunately be of lesser practical importance for forecasting *tropospheric* anomalies if they occur without a SSW? On the other hand, if  $E_1$ -type variability can be used to guide tropospheric forecasts even without a SSW occurring, that would be very useful information as well.

Again, I don't have any conclusive answers to the above questions, but it is probably worth pointing out that your results do appear to generally agree with some recent results that myself and a co-author recently published (Albers and Newman ERL Feb. 2021). In short, our results suggest that linearly predictable stratospheric anomalies are associated with enhanced tropospheric predictive skill of the NAO. Of relevance here, is the fact that our results have some interesting similarities to what you have found in your paper. For example, similar to your results, we find that strong downward propagating stratospheric NAM anomalies are generally associated with linear processes for lags as far back as 25-30 days prior to 'stratospheric NAM event onset'. Likewise, we also find that nonlinear

processes only become important 0 to 15 days prior to 'event onset' (denoted by stippling in our Fig. 1b). Interestingly, we were able to identify two types of dynamical modes (note, these are not EOFs), one single mode representing purely stratospheric processes (related to the NAM), and a second collection of modes representing coupled tropical tropospheric-stratospheric processes. In terms of subseasonal predictability, these modes account for a small fraction of overall NAO variance (see our Fig. 5c), which helps explain why subseasonal forecast skill is so low on average. We did not, however, provide any insight into which processes (purely stratospheric vs. tropical-stratospheric) are more important for subseasonal predictability (that is, we did not address the questions outlined in Domeisen et al. 2019 or Afargan-Gerstman and Domeisen 2020). On this note, does your  $E_1$ -EOF have any relationship to any forms of tropical variability?

In closing, you mention in your paper that you would like to explore the implications of your work in the context of actual subseasonal predictability. Given that you already have identified an EOF that you believe is important, one quick test you could do would be to project IFS hindcast data (or whatever your preferred S2S model is) onto your  $E_1$ -EOF and then calculate 'forecasts of opportunity' as periods when the forecasted  $E_1$ -EOF loading is particularly high. This would be a very rough way of identifying when the 'signal' part of a signal to noise calculation was particularly high, which typically equates to periods of higher forecast skill. If your  $E_1$ -EOF is identifying a 'skillful' portion of stratospheric variance, then these high loading periods may be associated with higher tropospheric skill (this type of 'signal' calculation is not as complete as computing the actual 'signal-to-noise ratio' as we did in our ERL paper, but it is easy to compute and seems to work reasonably well in some circumstances, e.g., Albers et al. WCD 2021).

One final minor comment...for the de la Cámara et al. paper that you reference, the 'de' and the 'la' are not capitalized.

Best regards,

John Albers

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